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SUMMARY OF PANEL DISCUSSIONS. RETIREMENT FOR CAUSE WORKSHOP HEL--ETC(U)
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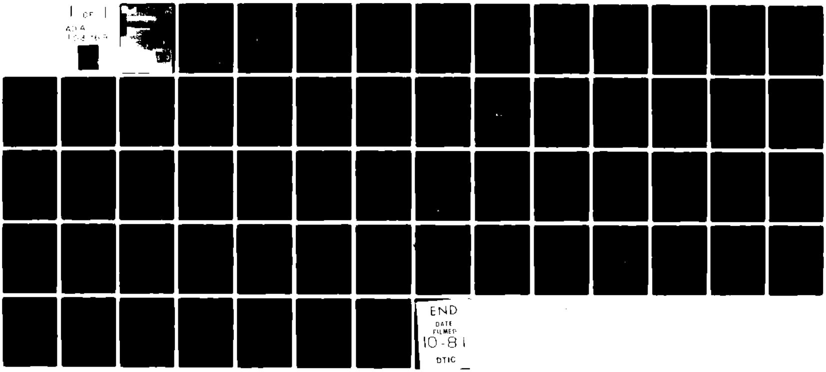

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SUMMARY OF PANEL DISCUSSIONS,

RETIREMENT FOR CAUSE WORKSHOP

Sponsored by

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Held at

Iowa State University,
Ames, Iowa, ~~50011~~

October 27-29, 1980

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FOREWORD

In this report are given the transcripts of panel summaries and discussions developed at a Workshop held at Iowa State University October 27-29, 1980. The subject of the Workshop was related to Retirement-for-Cause (RFC) strategies with particular emphasis placed upon the role of nondestructive evaluation (NDE) in such strategies.

The Workshop was composed of several parts. On the first day, a series of overview papers was presented which served to establish a common foundation for Workshop participants, who came from both the RFC and NDE communities. On the second day, the participants were divided into three groups to discuss and compare notes on three important subjects. One panel was asked to consider various operational topics in NDE of importance to RFC. These topics included NDE reliability, quantitative features of NDE (what data does one store?), standards, internal defects, benefits of automation, genericism, sensitivity questions, and questions of time and funding necessary to implement proper technology transfer steps. The second group addressed the subject of NDE measurement opportunities and "windows." Emphasis was placed upon three topics; they were measurement opportunities and evaluation for RFC, residual stresses, microstructure, fracture toughness, flaw sizing, and transducer improvements and signal processing. Finally, the third group considered topics related to failure prediction and accept/reject criteria. Included in the list were discussions related to the role of accept/reject criteria in driving NDE requirements, effects of accumulated damage and performance history, the utilization of extreme value statistics, decision display alternatives and, finally, liability considerations. The third half-day was devoted to the presentation of summaries of the panel discussions. These summaries evoked considerable discussion among the Workshop participants. Transcripts of these summaries and the ensuing discussion form the principal content of this report.

It was the consensus of opinion of the Workshop participants that the Workshop provided a needed and desirable interdisciplinary forum for technical information exchange. This exchange should be continued; many participants expressed the feeling that this kind of forum should be provided on a more frequent basis to help promote technology transfer.

The Workshop organizers wish to express their thanks to the Defense Advanced Research Projects Agency for sponsoring the Workshop, to the speakers who provided necessary information, to the participants, and to members of the Iowa State University staff.

Donald O. Thompson
Principal Investigator

RETIREMENT FOR CAUSE WORKSHOP

PROGRAM

Monday, October 27, 1980

R.R. Rowand, Chairman

8:00	Registration
8:30	Welcoming Remarks - R.S. Hansen, Ames Laboratory
8:45	RFC: History and Purpose - W. Reimann, AFML
9:30	DARPA RFC Goals and Objectives - M.J. Buckley, DARPA
9:50	Workshop Plans and Purpose - D.O. Thompson, Ames Laboratory
10:15	Break
10:30	Application of Engine Component RFC - J.A. Harris, Pratt & Whitney
11:00	NDE System Requirements - J. Doherty, Pratt & Whitney
11:25	Importance of Probability of Detection and Reliability of Measurement Requirements -- J. Moyzis, AFML
11:50	Design Trends in Turbine Engines - T.G. Fecke, AFPL
12:30	Lunch

D.O. Thompson, Chairman

1:30	Flaw Characteristics in Turbine Engine Materials - C.H. Wells, Southwest Research Institute
2:15	Role of Materials Failure Modes in RFC - O. Buck, Ames Laboratory
3:00	Break
3:15	Impact of RFC Requirements on Nondestructive Measurement Techniques - R.B. Thompson, Ames Laboratory
4:00	Failure Prediction and Accept/Reject Criteria for RFC - C. Rau, Failure Analysis Associates
4:45	Discussion
5:30	Adjourn

Tuesday, October 28, 1980

8:30 a.m.-5:00 p.m.	Working groups will be formed to examine key topics in RFC strategies. The group sessions will focus primarily upon the nondestructive measurement and accept/reject criteria aspects of RFC.
5:30-9:30 p.m.	Social Hour and Banquet

Wednesday, October 29, 1980

8:30 a.m.-1:00 p.m.	This final half day of the workshop will be dedicated to summarizing and discussing the recommendations of the working groups.

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SUMMARY OF PANEL DISCUSSIONS

DARPA/AFML
RETIREMENT FOR CAUSE WORKSHOP

Iowa State University and
Ames Laboratory, USDOE

Panel One

Nondestructive Evaluation Topics

Presentor: J.S. Cargill

Panel Members

J.S. Cargill (Chairman)	V. Panhuise
J.D. Achenbach	L.W. Schmerr
R.C. Addison	J.B. Sharkey
L. Adler	J. Moyzis
A. Bahr	K. Shimman
D. Birx	A.L. Thompson
A. Greer	V.V. Varadan
T. Kincaid	

J.S. Cargill: Attempting to summarize what transpired over those few hours yesterday is rather difficult, but I think that the input of several different people allowed us to get a more or less objective opinion on what the points of agreement were. First we ought to address the key topics, as shown in Figure 1.

KEY TOPICS

Suggested Points for Discussion:

- NDE Reliability
- Quantitative Aspects (and what do you store?)
- Standards
- Internal Defects (do we need to look for them? Where?)
- Benefits of Automation
- Genericism
- Buckley's questions:
  1. How do we get the sensitivity we need?
  2. Is there adequate time and funding to do the job?
  3. How can we improve communication in the NDE community?

Figure 1

The first point is one which we could probably have argued for at least a day and a half. I think we did make some headway on reliability, though. The second point was quantitative aspects of NDE and, assuming that you do quantitative NDE, what do you store? Standards were addressed from the standpoint of calibration standards and also verification of instrumentation and the retirement for cause inspection system. Next, do we need to address internal defects or not, and where are they if we do need to address them? Benefits of automation is something that has become fairly obvious as a necessity for retirement for cause inspection. Whatever evolves will probably have a large degree of automation. Genericism is certainly worth discussion, if for no other reason than it's been a buzz word around here for quite a while. So perhaps definition of genericism requires attention.

The question raised by M. Buckley in his opening remarks regarding how you get to the sensitivity you need is important; do you just increase the system gain so that you essentially reject everything that goes through? Obviously we can't do that. Mike's question about adequate funding and time to perform the job needs consideration. Finally, communications problems in the NDE community must be considered. I wouldn't say that is just the NDE community; I think it is all the technology going into retirement for cause in general.

Before I came out here, I spent quite a bit of time trying to determine exactly why we were getting together. Yesterday one point really hit home--as we look back on communication between what has been happening in quantitative NDE and the retirement for cause contractors, there has been inadequate communication ever since we have really become involved in retirement for cause in a big way. If nothing else, I think that alone justifies this meeting. I think we made some headway in that area, too, over the last two days. These items are given in Figure 2. As we began to address that subject, one of

SUGGESTION FOR NDE MEETINGS:

- More exchange from applications engineers to research scientists
- Workshops
- Technical summaries

Figure 2

the things that came up was the annual NDE meeting and we made several suggestions, which I think are all valid. One of the suggestions was that rather

than have discussion centering only on research and the researchers telling each other and the applications people what they have been doing, there should be some technology transfer or technical discussion in the other direction. The researchers would like to know what we are doing and what our problems are. Now, I'm not sure what the balance ought to be here. Perhaps we should not get into something like this at the annual NDE meeting itself, but I think we need a time once or twice a year when the researchers can find out what our problems are and what is on our minds.

The second point, and I think this became apparent to everyone yesterday, is that the formal presentations take you just so far. Workshops can get a lot more done in conjunction with the formal presentations. We get a lot of thought-provoking discussion in workshops, and we need more of these.

The third item, and I thought this was also a good suggestion, is that when the researchers get done with their presentations, we applications people need someone to translate and tell us what it all means so that we can be assured that there is a way to take that technology and translate it into something we can use: what we need versus what they have and how long it will take to make that transfer.

We also addressed the idea of technology development, as summarized in Figure 3. This was a rough one, and I think Frank Taylor had an interesting

TECHNOLOGY DEVELOPMENT

The vehicles are limited:

- 6.2 Programs
- Industry will support on "As Needed" Basis

Figure 3

point yesterday. He felt that it is partly the fault of the government that the entrepreneurial spirit may be lacking somewhat nowadays compared to what it was a decade ago. As far as taking technology from the research scientist level and developing and implementing that, the vehicles are quite limited now to the point where we are talking about 6.2 type programs. Also, and industry will get into this, there is the profit motive involved almost every time, so there has to be a fairly short term payoff. In a lot of cases, we don't think that is healthy.

I would have liked to have had more solutions to the problem, but this is really all we could come up with right now; I think that is possibly a sad state of affairs. Several points of general agreement were reached yesterday. These are listed in Figure 4.

GENERAL POINTS OF AGREEMENT
(There Weren't Many)

- Radiography is Out for RFC.
- Basic Research → Implementation Takes at Least 10 Years.
- Communication Gap.
- Nobody Has Enough Time or Money.
- Amplitude Information Alone Insufficient.
- FBH Standards Won't Do for Validation.

Figure 4

We got the radiography out of the way in a hurry. The second point was--this required a little bit of argument, but I think we narrowed it down--that you are not going to do much of anything from the good research idea to the point of implementation in less than a decade. It is readily apparent that there is a communication gap within the NDE community and the retirement for cause technology in general.

Everyone complains that they don't have enough time or money to do the job adequately. Retirement for cause inspections and accept/reject criteria are going to have to be based on a lot more than amplitude information alone or they won't work. This is something that we have seen in practice already. We are going to have to become a lot smarter. Another truism is that flat bottom hole standards obviously are not going to suffice for validation.

The first item on the agenda was to rank these features (Figure 5) of

IMPORTANCE OF NDE MEASUREMENTS
As Related to RFC

1. Flaw Detection Reliability
   Degree of Degradation in Difficult Geometries
2. Flaw Sizing
3. Procedures for Tracking and Recording Inspections

Figure 5

retirement for cause. We agree that all of these items are essential, so it is probably ridiculous to try to rank them. But we felt that no matter what you've got, if you don't know what it is, you can't go much further. So reliability is of the utmost importance. As I mentioned before, we are not going anywhere unless we have quantitative NDE, if for no other reason than (as several people pointed out the other day) the Probability of Detection curves are not vertical. If we want to have good reliability on a particular size defect, we are going to have to be able to sort out that size defect from a lot of stuff that is smaller and that we don't want to reject or we are just automatically going to ditch everything. Procedures for tracking and recording are still essential. It is going to have to be done or we can't do retirement for cause. If we had to rank them, that would come out number 3, perhaps because we feel more comfortable with number 3 than with the first two.

If we go back and look at the question of validation, we come up with three suggestions for ways to validate the retirement for cause inspection system, as given in Figure 6. If we are going to assess the system on cracked

SYSTEM VALIDATION

Suggestions:

1. Experimental - Prototype to PWA during verification testing
2. Analytical/Experimental Predictive Model - Calculate orientation effects, etc.
3. Continuing On-Line Verification

Figure 6

F-100 parts, that is really going to be the only way to do it. We are going to have to do some assessments during the verification testing of the DARPA/ Pratt-Whitney program. The second point is an analytical/experimental approach. The reason the analytics have to be there is because there is no way we can do the whole job on an experimental basis. We are going to have to fine tune the analytical models with experimental data to increase our confidence. We will, of course, continue with on-line verification, as it is an after-the-fact type thing. After it is in use, we will obtain continual feedback to determine how well we are doing. These are the three key features on system validation.

It became apparent yesterday that there was a lot of conjecture going on regarding quantitative NDE; we ought to have this, we ought to have that. But it was apparent that we still haven't hung our hats on anything. We still need to determine which quantitative methods ought to be used, and we are going to have to make up our minds pretty soon because we don't have that much time to complete development. Many options have shown laboratory feasibility; specific problems need to be identified and prioritized in a way that represents a consensus opinion. These options need to be evaluated against the prioritized list. Some items are listed in Figure 7. The

CONCERN OVER QNDE
Method Still Must be Selected Utilizing New Technology Showing Feasibility.
Examples Include: B-Scan, C-Scan, Born Inversion Technique, Long Wavelength, Pattern Recognition, Frequency Analyses, Elastic Wave Reliability Modeling, Signal Processing Advances, etc.
Note: "RFCI System Design" to address this.

Figure 7

retirement for cause inspection system design program addresses these to a large degree; some of these decisions should lie with the NDE people. The rest of us can't ignore them. This is something that is very important and we need a near-term decision on it.

Tracking and data recording methods (Figure 8) are dependent upon items

TRACKING AND RECORDING METHODS
Dependent upon:
• Quantitative Methods Selected • Future Goals

Figure 8

given in Figure 7. You can't really adequately define what you are doing in the tracking and recording until you determine which method you are using.

Questions of standards also entered our discussions, and centered on topics given in Figure 9. We decided that standards ought to be grouped in three ways, the first being equipment set-up standards. This is just to show that the equipment is functioning the same from day-to-day or the equipment in Florida is operating the same as the equipment in San Antonio, the same as

STANDARDS
Three Types:
1. Repeatability/Equipment Set-Up Standards FBH type ok.
2. Verification Standards - need real defects.
3. Preliminary Standards - combined artificial and real.

Figure 9

Dayton or wherever else we may have similar systems. In that case flat bottom hole standards, any reproducible standard, may be the best way to go. Initial check-out of the system is an intermediate step between 1 and 2, on the other hand. We need some kind of preliminary standard to check out the system, and in that case we don't need real defects. We ought to have some real defects in there, but in order to fine-tune the equipment, to work with the bread board, Elox slots, etc., simulated defects would be adequate. Cost and time are important considerations here. We'd certainly like to have real fatigue cracks, but realistically we can't. For verification, though, I think everyone agrees that we need real defects. The only way we're going to convince anyone the system works is to demonstrate it on the real thing; otherwise it will never be implemented.

The question came up as to whether we should have 100% coverage of each component; i.e., 100% volumetric inspection (Figure 10). That is probably

100% COVERAGE OF COMPONENT?
Use Priority Analyses to Weight Stress Areas--Probability of Failure and NDE Required

Figure 10

unrealistic, and I think we can do a better job than that using the design system. We can come up with a priority analysis to determine which areas ought to receive 100% inspection within those areas and which areas we can let go for now, and in which areas the probability of developing a defect that could give us trouble is low enough that we don't have to worry about it. In some cases that could be significant because we could spend a lot of money developing a technique for inspecting an area that has a one in a million chance of developing a fatigue crack.

Regarding system design (Figure 11), we have a number of options in

SYSTEM DESIGN

Module to Perform on — One type of Component or One type of NDE

- RFCI Design Program
  Determine Groundwork
- Man-Tech Program
  Conduct Cost Trade-Off Study

Figure 11

developing the modules. One option is to have each module perform a certain type of NDE; another would be to have the module inspect a certain type of component. We think a lot of that depends on the cost trade-offs. I'm sure anyone involved in the Man-Tech program would agree with that. We also think that it ought to be considered within the realm of the inspection system design program to lay the groundwork for that cost study. That is not a lengthy task, and would establish a set of guidelines that the Man-Tech program could follow in the cost trade-off study.

If we consider automation--which again is going to be a key feature in retirement for cause inspection--we consider anticipated improvements, as shown in Figure 12. There are a number of things that obviously require improvement--

AUTOMATION

Anticipated improvements in

- Subjectivity--i.e., human error
- Thru-put capabilities
- Repeatibility/Reliability
- Precise Manipulation--(E.C.)
- Scan Paths--(UT)

Figure 12

elimination of human error, for one. Everyone talks about that but other areas, such as through-put capabilities, repeatability of the inspection, consistency of scan paths, preciseness of the manipulations (not necessarily from only an eddy current standpoint) also need improvement. The qualifier here is that we will see a large improvement in inspection reliability and repeatability, given that what we are looking at are all cracks. Automation alone isn't going to allow us to do the whole job. By that I mean that if we have scratches, dings, dirt on the parts, anything else that can give us a false indication, we can't expect just automating the system to give us a huge increase in reliability and allow us to sort all the good from the bad.

As far as what ought to be automated--and we have had a lot of discussion on this even since the committee met yesterday--it is obvious that ultrasonic tests and eddy current could be automated fully within the realm of feasibility. When we consider fluorescent penetrant inspection and the cost involved in fully automating that for discs, it does not appear likely that the automation will be completed in time, so we feel the inspector's subjectivity is going to have to remain in the fluorescent penetrant system or module. We still feel that the fluorescent penetrant inspection ought to take place, if for no other reason than that the defect that gets you in the long run is the one that you didn't plan for. We need a full-coverage inspection for the full field involved in retirement for cause.

Last but not least, this type of meeting is something that doesn't happen often enough. I think we made some headway yesterday. We feel that it would be quite worthwhile to have meetings of this kind, at least on an annual basis, perhaps as a subset of the summer meetings. Sitting back and listening to someone give a formal presentation does not solve our problems. We need more opportunities for dialogue before we can communicate effectively.

CONSENSUS

WE SHOULD DO THIS MORE OFTEN.

Figure 13

That concludes the summary. Go ahead and address any questions now.

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Panel One

DISCUSSION

J. Harris, Pratt-Whitney Aircraft: I detected a little conflict in some of the answers that the group arrived at, specifically, 100% inspection. Can you clarify?

J.S. Cargill: We were talking about 100% volumetric inspection, the total ultrasonic inspection for internal defects, say, in rim locations on the discs where it would be almost impossible to develop 100% volumetric inspection. And the design tells us, as you well know, that we don't have to worry about the internal defect out in the rim location as much as we do in the web and bore, which are simpler.

J. Harris: Your definition then was 100% volume as opposed to 100% surface condition?

J.S. Cargill: Yes.

J. Harris: Did you also consider in your discussion that we can do the most amazing inspections and the part still isn't any good because there is a lug missing on the disc and it will not go back in the engine, or the spline was gone?

J.S. Cargill: I think that follows in the dimensional check.

C. Rau, Failure Analysis Associates: Steve, if you are going to your validation procedures and you concluded that experimental validation on the actual prototype cracked parts was the only real way to go--I concur with that, by the way--did you discuss how much we can deviate from the expected cracking rate in our evaluation procedures and still hope to have a realistic evaluation? In other words, do we expect to have one in ten thousand cracked? Can we go with a test line which has one in ten cracked and have any hope of really evaluating the true reliability of the inspection?

J.S. Cargill: There was some discussion on that, and I think it was generally conceded that we can't build the whole show just on that verification testing on a few components, but it is certainly an essential part of the reliability estimates. We don't think that we will have enough cracks and enough parts from the spin test at Pratt-Whitney to do the whole job on reliability determinations, but it is a key step.

C. Rau: That is not quite what I am asking. I realize you won't have many cracks. My comment is that we need a lot of uncracked ones being run

through the same inspection procedure. If we expect only one in ten thousand to actually be cracked, can we hope to get any reasonable evaluation if, in our procedure, we test ten parts in which three are cracked?

J.S. Cargill: This is something we are always up against in the reliability programs, Charlie. If nothing else, it is always a cost problem. You can never come up with a sufficient ratio of uncracked to cracked parts to go out in the field and subjectively say yes, this reliability transfers to that. However, since we are talking automation, it shouldn't make much difference if we are talking about the eddy currents and the ultrasonics.

F. Taylor, Systems Research Laboratories: One of the key problems here is that you won't be able to prove what the equipment is really doing. You can't tell what it is doing until you try it out in the real world where it will be used. People are going to use the equipment; maintenance is involved. How well they adhere to procedure, plus the fact that you will have real parts coming through the system that will require going around a real engine, are vital considerations. This is really the crux. You cannot evaluate the system. You can't tell what you are going to do for sure until you are doing it. It has to be verified in place. We won't know until we have a chance to evaluate, and that is going to be an ongoing procedure which will begin in 1985. Up to then, all we are doing is taking the best guess we can with the data we have. They are really not going to be simulating the environment in which it will work.

J.S. Cargill: Inspection reliability is always an estimate. It is just a matter of starting with a rough estimate and trying to polish it as you go along.

R. Rowand, Air Force Materials Laboratory: I think it is worth pointing out that here in this room is a collection of engineers and scientists, and we all like automation. It is a very nice word. It is going to do a lot of great things for us. But I think we must be very careful to remember, as we progress in any kind of program like this, that we are dealing with a factory environment, and that we have to worry about human engineering, human factors and designing equipment, etc. I think we should probably go on record as saying that any future workshops we have in this area probably should begin to get some human factors type of people involved and look at what we are doing and how it fits. It is a subtle difference, but it is very important. We get there without involving such people, but sometimes we put the wrong colored knobs on or the data input approach is not as easy to work with as it should be.

The second point I wanted to mention was that the manufacturing technology program, of course, is going with the available state of the art, as you summarized.

As you know, I am *not* very pro-radiography, but it may turn out that with some components we will have to proceed with very carefully selected microphoto techniques or CAT scans to get the kind of resolution that we would want and probably in a non-automated kind of operation.

The other item worth pointing out is that we have been primarily talking about the Air Force. We have many other subjects that also use gas turbine engines, and eventually they will probably become involved and maybe we can even steal some of their money. We are trying to build a program which is modular in nature, saying that if we pick all of the right approaches, including the systems approach, to mechanical hardware, as we get new technology which offers better resolution or definition of flaws we will be able to just add additional modules.

J.S. Cargill: Those were all very valid points. I'd like to say that as far as the human engineering factor is concerned, it is being considered by the addition of Giffels Associates in the system design program. They have done quite a bit of human engineering work.

W. Reimann, Air Force Materials Laboratory: I wanted to move for a moment from the sublime to the ridiculous, if you like, and ask you whether you considered some other points which came up in our discussion in the accept/reject area in connection with the inspection. What it comes down to is some of the concerns of the human factor sneaking back in again, even in an automated system. Let me give you an example. You can develop fully automated machinery which does all of the number processing, makes all of the decisions, and finally turns on a green or a red light. If it is a green light it goes into the good pile, if it's a red light it goes into the bad pile, except when the operator makes a mistake and puts one from the red pile into the green pile. Did you consider this aspect of the problem?

Secondly, in connection with the question of continual verification and standardization and so on, Jack Harris brought up a good point in our conversation yesterday. He said that one of the things we could do, for example, is to take some real hardware and induce some force, then we could use these as test components so that every now and then we whiz them through the machine and see if they are still giving the answer. What if it gives the wrong answer and accepts that component? All of a sudden you've got a good one that you know is flawed in a stack of good ones. Did you address that sort of question?

J.S. Cargill: We have given that some thought, and one of the ideas we came up with was to introduce a small amount of radioactive material in a small

number of them so you could use a Geiger counter to call that disc out later on. However, it is not going to be 100% reliable. Nothing is. There is a possibility that a component could sneak through. It is a matter, again, of whether it is worth it to you to know that a system is in complete readiness all the time to run those components through.

W. Reimann: Maybe one approach to that is when you send components through, you deliberately knock out the tines or something so that you couldn't possibly run it through.

J.S. Cargill: Depending on how much prior human input is in the system, of course.

F. Taylor: I'd like to respond to the first of Wally's questions. The human engineering portion of designing a product for a system is really the responsibility of the designer. The engineer should take into consideration the human engineering factor, the human interface with the machine. A product or a system, when it gets out to the final customer, is not a success unless that user, that customer, is satisfied with the machine. He makes it work. The interface has to be such that they are glad they got it and the operators prefer to use it. Otherwise the design engineer has failed in his job. I know that traditionally this has been a very difficult point because we have a lot of research scientists and engineers who want something neat in their laboratory so when they design something they design it for these high caliber people to operate. But you can't do that; you must design equipment for the operator.

C. Rau: I was still trying to follow up on my original question. Maybe it belongs in the accept/reject discussion. I agree with everything Frank said with regard to what you really can and cannot do. What I'm a little concerned about is that we will get to 1985 and 1986 and 1987, and even though we once get on-line, if we don't get a whole lot of cracked parts we still don't have a vehicle with which to start developing the inspection reliability with the kind of quantitative basis we really need to doing anything close to an optimum RFC accept/reject procedure. So I think we have to address the very difficult problem of doing a better job of estimating what that reliability is going to be before we get to the bottom line, or some time during the early stages of implementation, because even if we get some signals we are not going to be cutting up all the good ones to see how many we may have missed.

R.B. Thompson, Ames Laboratory: This is one of the things that we discussed in the panel yesterday, and it appears that we have now developed a basis of theoretical solutions to describe this. The question is, can we cast these

in a sufficiently accurate engineering context? We can even use these to model the performance of the system as it is being built. There is always the question, do your models sufficiently accurately predict a real crack rather than some mathematical idealization? I think we have at least the foundation for doing those kinds of studies in the next few years rather than waiting until the system is totally built.

B. Tittmann, Rockwell International Science Center: I was very interested in your discussion of the standards. You mentioned calibration standards for the equipment as well as verification standards. Are there not, at this time, such verification standards available, or is there a formalized plan to develop these, and would they be available to the research scientists?

J.S. Cargill: Do you mean as far as actual defects are concerned?

B. Tittmann: Yes, real defects.

J.S. Cargill: This really gets back into what we have just been discussing for validation. The verification standards and validation go hand in hand. There is a very limited number of cracked engine parts around at any time just because they are so valuable. We need them, and they shouldn't be specimens, either. They should be real parts. I agree; that is the hard part. I think they ought to be available to the researchers and we need more real parts available for reliability estimates.

B. Tittmann: What would be a possible source for such parts? Would it be the Air Force, or Pratt-Whitney?

J.S. Cargill: The Air Force and, to a certain extent, the engine manufacturers. That about covers it, really. Charlie, would you have a suggestion on how to build up the repertoire of specimens?

C. Rau: Only the obvious. I think we do have to use real cracks and real parts, and we can generate the parts in spin pits and Ferris wheels. We can generate cracks in the regions where we would like to have them, or where we think we are going to get them. We could attempt to run them through the system or modules or some sets of the system before we actually get the full system operational. Granted, there will be differences. Dimensional tolerances have changed and there will be dirt and oxides. It will be a lot closer, though, than trying to use our analytical models in time to really affect the inspection reliability the way I perceive we are going to need it.

R.B. Thompson: Perhaps I made an incomplete statement. I agree that the models are certainly not ready to do that whole job. If we are clever we might be able to use them to piece between the limited experiments that we

can do because we can never do enough experiments to cover all possible situations. So there has to be a very close approximation between models which will be approximate and experiments which will be limited.

J.S. Cargill: It all boils down to cost and time again, really. It costs a hundred thousand dollars to run any kind of spin test.

J. Harris: In regard to your question, Bernie, I don't think there is anyone in this room who would not say that actual inspection of cracked components is the thing to do, and it would be great if we could get everyone his own parts. One of the constraints we are going to have is that you are all not going to have cracked parts to work with. They just won't be there. If we get a cracked part we are going to exercise the devil out of it. On top of that, Steve just mentioned that it costs something in excess of a hundred thousand dollars to run a complete laboratory-type spin test. Probably the part itself is going to cost twenty thousand dollars, so you are up to a hundred and twenty thousand dollars a part. There just isn't enough time and money in the world to be able to supply cracked parts to everyone. I would not like to raise any false optimism; they just won't be there.

J.S. Cargill: It may not be all that bleak, because we are not the only engine manufacturer; some of the others may want to participate.

J. Harris: We are not the only ones, I'll agree. But if we get a cracked component, we keep it in the house.

W. Reimann: I think what Jack said about the F-100 is very true. The number of cracked parts we are likely to have in the time period is going to be pretty small, and as Jack said, there are parts that are going to be in high demand. The F-100 program office is interested in those parts for running engine tests. We, of course, in our program are interested in running engine tests on those components. So the idea of deliberately flawing a component is an anathema right now because we have something like an eight-month delay time. However, I tend to feel that a disc is a disc is a disc, and I believe that the Air Force, for example, has the world's largest supply of cracked TF-33 turbine discs. A crack is a crack and a disc is a disc. I think there are options.

F. Taylor: Just to put the proper perspective on things, in order to fully verify the systems empirically, experimentally, and with enough statistical data that you can make a prediction, you would need fatigue cracks of various sizes, of various shapes, orientation, in every location in all 21 discs that we are talking about. First, you couldn't even draw them and write

sizes to get the points on a curve that you would need to confirm. We are talking about a program that would probably cost a million dollars or more to verify the system. The only way we are going to get this is by using models. That's all we can do. All we are going to have is a good guess. It may not even have statistical significance. We are going to have at least an inkling that it looks like it is going to work. The whole program is about to establish the capabilities.

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Panel Two

NDE Measurement Opportunities and Windows

Presentor: O. Buck

Panel Members

B. Auld	J. McClelland
O. Buck	B. Tittmann
J. Doherty	J. Moyzis
R. Elsley	C. Wells
D. Finnemore	D. Thompson
L. Kessler	S. Bennett
K. Lakin	D. Birx

O. Buck: We realized pretty early in the game that the types of questions that had been outlined for the panel are techniques that are probably not going to be implemented before 1985--perhaps later, perhaps never. I must say I learned a lot of things that I had not known before. Let me just try to go through the types of subject areas we tried to discuss.

As shown in Figure 1, subject number one dealt with measurement oppor-

SUBJECT AREAS DISCUSSED

1. Measurement Opportunities and Evaluation for RFC
2. Residual Stresses
   Microstructure
   Fracture Toughness
   Flaw sizing < 100μm
3. Transducer Improvement
   Signal Processing

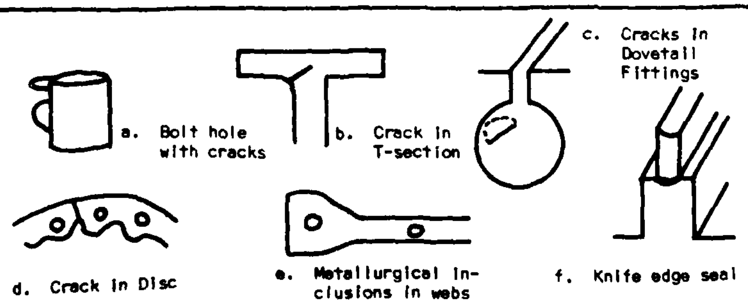
Figure 1

tunities and evaluation for RFC. As you will see later, certain feasibility studies have been performed in the laboratory; these studies are now ready for implementation on an engine disc. We felt it worthwhile to take a look at these "far-out" techniques and see whether they may be ready by the 1990's. Furthermore, we wanted to determine what we would have to do to accelerate the implementation if the NDE community agrees that they show promise in the field. As a number two item we discussed residual stresses, which is a very interesting and important subject that is not considered in present day RFC considerations. We did not talk too much about microstructural aspects of RFC. Similarly, we did not talk much about fracture toughness and whether it is

necessary to monitor this quantity. The item "flaw sizing" has to do with the number one item. In number three we talked about what should be done in the area of transducer improvement and signal processing.

Very early in our discussion, Joe Moyzis pointed out that there are probably three types of flaws, and that we should categorize flaws in an engine disc according to this scheme. They are surface flaws, near surface flaws and bulk flaws. What do we actually mean by near-surface flaws? We came up with a definition that says that a near-surface flaw is one that has a diameter d and a distance a away from the surface. If a/d is about 1, we will call this flaw a near-surface flaw. At the present time these near-surface flaws could be missed during quality inspection, before the engine is delivered. As the engine goes into service the near-surface flaws will break through the surface and form a regular surface flaw. Although there is a good chance that near-surface flaws become surface flaws in service, in our discussions it became apparent that no one really had a good answer as to their quantitative sizing by NDE techniques. We estimated that typical dimensions for these flaws are a 100μm and d $\approx$ 20-50μm. It is interesting to note that residual stresses, due to machining, decay out at about 50μm depth. Furthermore, the stress concentration factor due to such a flaw drops off rapidly with distance away from the flaw and is basically zero at a distance of 50μm. Therefore the above definition for a near-surface flaw makes this flaw quite special in that the free surface shows a strong interaction with the flaw.

On Monday, a number of critical locations on a disc were discussed. These are schematically shown in Figure 2. Also, a statement was made that



1. Surface cracks of 100μm ($\approx$5 mils) should be detected reliably.
2. In RFC near surface cracks become true surface cracks in service.
3. Large area scanning for unexpected events important! (40 mils cracks)

Figure 2

surface cracks of about 100μm ($\gtrsim$5 mils) should be detected reliably. Near-surface flaws at these locations probably become true surface flaws in service. It would be interesting to determine how long it takes to initiate the near-surface flaws. Would initiation take place during the design life of the disc? I don't think we can answer this question at the present time since residual stress effects may affect the initiation of near-surface cracks strongly. Another good question was raised: Should we forget about monitoring of surface flaws in areas other than these "critical" areas? We came to the conclusion that the answer is no. A large area scanning for unexpected events is important. A flaw of 40 mils in length in a "noncritical" area is as bad as a 5 mil flaw in the "critical" area.

We devoted a fair amount of time to what kind of new inspection techniques are on the horizon, e.g., those that are farther out than quantitative ultrasonics. Jim Doherty and Joe Moyzis pointed out that most of the techniques that have shown laboratory feasibility will not make it by 1985. I think it was the consensus of the panel, however, that we should list them anyway. The

NEW NDE TECHNIQUES
(1985 or Beyond)

Technique	Surface Flaw (a)	Subsurface Flaw (b)	Feasibility	Probability of Success
Photoacoustics	✓	✓	Yes	Good for (a), (b), and large area scans
SLAM	✓	✓	Yes	Good for (a), (b), and large area scans
SAW NDE				
1. Harm. Gen.	✓	?	Yes	Large area scan
2. Mode Conv.	✓	✓	Yes	Good for (a), (b), and large area scans
3. Imaging	✓	✓	Yes	Good for (a), perhaps (b), and large area scans
Low F. Eddy Arrays	✓	✓	Yes(?)	Good for (a), (b), and large area scans
High F. Eddy Yig + Others	✓	No	Yes(?)	Good for (a) and <u>small</u> area scans
Capac. Probes	✓	No	Yes	
Positron ANN	No	No	plastic zone	utility?
Exo El. Em.	No	No	plastic zone	utility?
Acoust. Em.	✓	✓	Yes	spin pit?
Gauge	Yes	No	Yes	Possible application
Acoustic Q	?	?	Yes	Possible application to localized areas
Optical Methods				
1. Speckle	✓	No	possible	large area scans
2. Corr. Int.	✓	No	Yes	large area scans

Figure 3

result is shown in Figure 3. We rate the techniques with respect to their ability to detect surface flaws and near-surface flaws, and whether the techniques seem to be feasible. Our definition of feasibility is a demonstration of success in a laboratory environment. The last column in Figure 3 refers to potential field applications of these techniques at some time after 1985.

The photoacoustic effort is now under investigation. We came to the conclusion that it may prove useful to detect surface and near-surface flaws as well as large area scans. The SLAM (scanning laser acoustic microscope) certainly has shown great potential for detection of surface and near-surface flaws in scans.

The next item we discussed was "Surface Acoustic Wave" (SAW) NDE. We discussed several promising techniques: harmonic generation, mode conversion, and imaging. Surface flaw detection capabilities have been demonstrated. SAW NDE is certainly very promising for large-area scanning. As everyone probably knows, surface acoustic waves are useful to test curved surfaces. In our discussion of low frequency eddy current probes we came to the conclusion that it may be possible to develop arrays for fast and automatic scanning. Single, low frequency probes are in use in the field already, which gives us reason to believe that arrays should be useful.

High frequency eddy current probes was the next item discussed. Near-surface flaws probably may not be detected. Surface flaws, on the other hand, have been observed. Some of the probes are small, so that it seems feasible to inspect areas that are hard to inspect with other methods, such as corners and holes with a small diameter.

Continuing with electromagnetic probes, we briefly discussed capacitance probes. It is very unlikely that this technique has a potential to detect near-surface flaws. Positron annihilation is one of the more exotic techniques and is frequently mentioned as a possible candidate for detection of fatigue damage. My opinion is that surface and near-surface flaws cannot be detected using positron annihilation. However, plastic zones at the crack tip have been mapped out. Positron annihilation is sensitive to dislocation rearrangements, but I am not sure that this is important for RFC. The same is true for the so-called exo-electron emission technique. Again, plastic zones could be made visible. Utility for RFC, however, is very unlikely in that high vacuum is required.

Acoustic emission is another acoustic technique we talked about briefly. An external stress would be required to trigger a signal at a surface or near-surface flaw. It is hard to imagine, however, that the Air Force's Logistics Commands would allow us to apply a stress to engine discs. The only way we could see applications of acoustic emission may be in a spin pit test. In such a test, however, other difficulties come about. A spin pit has a very noisy environment and it may be impossible to select the important signals out of the background.

Some of us were aware of the Air Force's efforts to use mechanical gauges to monitor aircraft frames. Use is made of a precracked mechanical gauge mounted to the frame. By means of a "transfer function" it is feasible to draw conclusions about the growth of a crack at a critical, inaccessible location on the frame. The question is, is there any way to put a gauge on an engine? It may be useful to use the acoustic quality factor Q of a disc to monitor the state of the fatigue damage in a disc. Q has been used for many years in quality assurance; its very unsophisticated version is the famous "coin tapping" method. Certainly one could do much better now to determine Q with modern electronics instead of the human ear. If we have to detect a five mil crack, however, I feel that even our best electronics may not be sensitive enough to determine a change in Q. The use of optical methods, such as speckles or correlation intensity methods, was suggested. In the latter case laser holography and optical correlation between successive pictures have been used. The method is very sensitive to slip band and microcrack formation. Certainly a difficulty is that one would have to store the image for comparison with the later state of damage in the disc. Another topic that we tried to address was the effect of residual stresses on RFC. Figure 4 summarizes these discussions.

RESIDUAL STRESS

1. Designer does not assume presence of residual stresses at present time.
2. Machinist's experience is important in fabrication.
3. Long range stresses are important to crack closure in initiation regime.

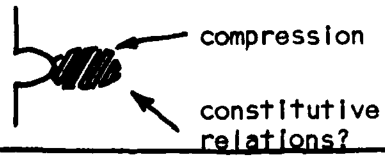


Figure 4

It was pointed out that at the present time the designer does not make any assumptions on the presence of residual stresses in an engine disc. Our feeling was that in the future this design philosophy may have to be changed. It is well known that residual stresses affect the fatigue life. A measurement of the residual stresses prior to putting the disc into service is not enough, however. Residual stresses change due to fatigue and therefore it may become necessary to monitor these changes on a routine basis. At present, the machinist's experience with specific materials is an important factor in the fabrication of the discs. If he knows his trade, favorable compressive residual stresses in the top surface layer will be introduced, probably giving rise to a certain conservation.

Certainly compressive residual stresses are necessary to keep existing cracks slightly closed, particularly in the crack initiation regime. These compressive residual stresses thus prevent the cracks from growing. Thus I believe that measurements of residual stresses after machining and during the service life of a part, as well as their use in design, may become a very important factor in a successful application of RFC.

As a last point, we discussed transducer development and signal processing. This is summarized in Figure 5.

TRANSDUCER DEVELOPMENT AND SIGNAL PROCESSING

- Conventional Transducers
  Radiation Pattern } for full flaw characterization
  Frequency Response
- Spatial Averaging to Reduce Grain Noise
- Time Averaging to Reduce Electronic Noise
  Signal/Noise $\propto \sqrt{\text{averages}}$
- Inversion Algorithms
  ka = 10 > Imaging
  ka = 2-10 Model Reconstruction--Born, Poffis
  ka = 1-5 ALN
  ka < 1 Long Wavelength

Figure 5

Conventional acoustic transducers should be better characterized as to their radiation patterns and frequency response. To us this seems important. Spatial averaging of acoustic signals to reduce grain noise should be done on a routine basis. Time averaging of signals to reduce electronic noise should

also be helpful for improved flaw detection. Last, but not least, inversion algorithms that have been developed recently, such as in the ARPA/AFML program on quantitative NDE, should be implemented in the RFC program as soon as possible.

That basically concludes what I had to say. I am afraid we came up with too many positive suggestions for the present RFC program. However, I believe that RFC will continue over and beyond 1985, once it is demonstrated that it is a useful concept.

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Panel Two

DISCUSSION

R. Rowand, Air Force Materials Laboratory: One thing has not been discussed this morning. When we are talking about the state of the art techniques or new techniques, the subject of cleaning prior to inspection and part preparation, where required, is quite important. In essence, this ties into Bernie Tittmann's comment earlier about standards, and I am going to show my bias about standards now, but standards are never standards--they never stay standards. Probably the worst example is a penetrant standard. Gradually, you fill up a crack if it's a tight crack, but you never get the dirt out of it. If you try to make quantitative comparisons between some standards and an actual defect you always find out that the crack was larger or smaller than your standard and often it is related to inadequate cleaning processes. This is another topic that we have to consider in all of our activities.

O. Buck: I agree. The subject did not come up in our discussion.

F. Taylor, Systems Research Laboratory: There are two areas here, probably mostly a matter of definition, that I think are important to clarify. What do we mean by automation? In Panel One at the very beginning we simply defined it to mean a replacement of the human brain in the decision-making process. In a corollary we concluded that RFC will not work if you have human decision making. This goes back also to automated FPI, where it may happen that the inspector is not fully alert. I think, regardless of whatever technique we are looking for, at the present time we are relying upon a human brain to make the decision, and that is not going to work. We need full automation, a replacement of the human brain.

R.B. Thompson, Ames Laboratory: I really just want to follow up on some comments that Dick made. One of the things that Steve didn't get a chance to cover was a discussion we had about a particular technique whose success, perhaps, somewhat depended on how well the part could be cleaned. This is an area of specifications I think the people developing the techniques don't have a good feel for. Some techniques certainly would work if you could get the surface in good condition, but God knows if the Air Force or the manufacturer would allow that to be the case. I think that this is an area where greater communication is needed. I don't know how to stipulate it.

O Buck: I might say that we discussed part of this problem, too, particularly with respect to parts rubbing against each other. What do you do in that case? This situation may be even worse than the cleaning procedure or inadequacies in cleaning procedures. If you are supposed to look for five mil cracks, I don't know the answer.

J. Moyzis, Air Force Materials Laboratory: I think you are talking more about problems for Panel One rather than Panel Two. If Panel Two really knew that what we were doing was trying to look at more exotic or advanced procedures, ones that we wouldn't be able to apply in near term but we could view as candidates to plug up holes in what we saw as problems in retirement for cause, we would have to address different problems. The only way cleaning would enter that kind of situation is if we could show, fundamentally, that a technique would be eliminated because of a cleaning difficulty. I don't think the cleaning issue addresses what we were talking about at all unless we could show that fundamental block.

R.B. Thompson: I agree. I was just replying to Dick's comment because I thought it was an important issue.

W. Reimann, Air Force Materials Laboratory: Otto, as I sat and watched your presentation, I had a couple of concerns. It came down to the definition of feasibility. There are a number of very exotic, very sophisticated approaches, and not being in the field at all I have no idea whether you are talking five years or 50 years to implement some of those systems. But I notice in a great many cases today they were checked as having established feasibility, and I guess I should remind you that feasibility means different things to different people. When you get down into the ALC's, you know, feasibility to those people means not that a Ph.D. sat down in the lab with a hand-built piece of equipment, measured something, and concluded that this is something that is a month away from having a full production unit. The point I want to make is that I would caution you in regard to the use of the word "feasibility" because it connotes different things.

O. Buck: Right. We defined the term "feasibility" as the laboratory demonstration of a technique that is useful to monitor the damage. We tried to break it down into several columns: feasibility in the laboratory, feasibility during the development stage, feasibility in field applications. We also tried to make an educated guess when we thought these goals could be achieved, assuming money would be available, and assuming enough manpower to really push those that look most promising. It was impossible, however, to get any consensus

on these dates. Basically, I agree with you, "feasibility" means different things to different people.

F. Taylor: My point is that feasibility is what the state of the art is. I think what you mean is too far back. You talked about 20 years from now. Some people think that is conservative; Jack thinks it's close to 20. But 10 years is a good average number. I think our definition has to be farther down the line; not only somebody did something, but somebody built something that actually works somewhere and it is doing the job. If we are going to look at 1985 for implementation, we have to start now.

R. Rowand: I guess to further add to my earlier comment, Bruce, maybe the academic community is doing the wrong thing. What we really need to develop is a DCD, which means "dirty crack detector." In other words, don't do any cleaning but find a way to find a crack that is completely filled with oxide.

J. Rose, University of Michigan: At the end you mentioned transducer development, and so this would be perhaps not quite so related. I think it is important in that we start talking about two octave bandwidths in our transducers. I notice that wasn't put down. I think that is going to be absolutely necessary. That is quite a bit down the road, perhaps. We have to get started, a couple of octaves in a couple of decades. We're really going to need it.

L. Kessler, Sonoscan: In the "potential opportunity" chart that we put down in Panel Two--I was involved in the panel--there is very little differentiation between many of the techniques. Perhaps we ought to look at it a little bit closer on another occasion and find these various levels of feasibility so the potential user--benefactors, if you will--can take a look at it.

O. Buck: Right. I would agree with that. That is a very good point. I would like to add that it may be worthwhile to perform round-robins to find the best techniques.

L. Kessler: I think even in the academic role we took yesterday, we would get a further differentiation than defining some level of feasibility.

O. Buck: Sure.

B. Tittmann, Rockwell International Science Center: I was involved in this panel, and the subject of surface preparation, surface changes, microstructure near the surface, came into discussion but we really didn't have a quorum to discuss that at great length. So this point is really addressed to the audience. In the long-range effort, how big a role do you see for a good characterization of that near-surface microstructure in terms of producing cracks in surface?

O. Buck: We came to the consensus that at present metallography is performed on the disc's microstructure; however, metallography is not repeated after the part has seen service. The opinion was that we probably don't have to worry about microstructural changes in the sense that phase transformations are induced or that grain growth occurs due to high temperature applications. We would probably not have to worry too much about such changes. We had another question that pointed in the same direction: Would we have to try to measure changes of the fracture toughness during the service life? Again, the answer was probably not, because we don't have a microstructural change. However, after that I talked to Cliff Wells, and one thing that worries both of us is the possibility of oxidation effects of the grain boundaries. Could this lead to changes from a transgranular initiation to an intergranular initiation, or vice versa, possibly having an effect on the initiation life? But I think we never came to a real conclusion on that question; it may be worthwhile to look into this in the future.

J. Harris, Pratt-Whitney: I'd like to make just an observation in that regard. Of course, people who make the components do look at long-term stability in their alloy programs. We do look very carefully at the microstructure on each, for example, of the high turbine parts. We have a process called 16-point check. That's surface. What we are talking about in retirement for cause, however, is that while we are satisfied that we are not going to have any drastic changes in the one lifetime, we have always been concerned with today--now we are talking about two or three lifetimes! I think it is a question that could be addressed in terms of priority. At present we are just ignoring it. We are saying that it is something to think about, and that is my observation.

O. Buck: That is interesting.

W. Reimann: I'd just like to follow that one step further. There is some precedence for doing microstructural evaluation of metallographic evaluation as a handy tool, if you want to think of it that way. We do have one engine in the Air Force which operates at an extremely high temperature condition where there is a concern for metallographic stability, and it is an accepted practice in that particular case to do replication of some spots on the surface and check the microstructure for grain boundary precipitation and so on. I agree with Jack's assessment at this point that it is probably a lower priority concern, and as we get experience with using these discs longer we may, in fact, decide that we do have to start doing something on that.

O. Buck: Could you tell us a little about the residual stress problem? Do you worry or not?

W. Reimann: I worry very much about the residual stress problem. I think all of us in the mechanics area are very much aware of the difference in residual life that residual stress can make in your calculations. Our problem, of course, is that we have no idea of how to get a handle on it or how to measure it, how reproducible it is, whether it stays there, etc. The point was made earlier, I think by Steve Cargill in his presentation, that our approach right now is to ignore residual stress. On the assumption that there is compressive residual stress, that is a very conservative approach. If it is tensile, we're in big trouble. The question of residual stress, in my mind at least, is a very key issue that we would dearly love to solve. Unfortunately, I don't know of any approach to it at this point, but I think it is a very key issue.

J.S. Cargill: I thought I heard you say earlier, Otto, that the state of the art techniques for aircraft technique components were limited to eddy current and fluorescent inspection.

O. Buck: That's the impression I got yesterday during our panel discussion.

J.S. Cargill: Was that from a production or field standpoint, or everything in general?

O. Buck: In the field where RFC will be executed; in other words, at the ALC.

J.S. Cargill: There is a surface wave inspection used on the fan blades of the F-100 in San Antonio; so it is being used, and even more so in production.

O. Buck: Thank you; I was unaware of that.

D.O. Thompson: I would like to make one comment before we proceed. It returns to this problem of availability of real samples. It crops up from the research point of view in several ways. One is, what is feasibility? Availability of samples of a real nature would help tremendously on that. It comes up in this question (Dick's question), what is a real surface in which to evaluate these things? Thirdly, it comes up in trying to make an assessment of new technologies which may have a role. Again, without the availability of real samples, from the research point of view one is forced into the mode of model-type flaws or model calculations, and so on. I think the overall effort would benefit if there were some convenient source of samples. They would move us one step forward toward closing some of the gaps between model flaws and flaws in which real answers have to be given.

C. Rau, Failure Analysis Associates: I'm not in the community directly, but I believe that statement is a bit of a cop-out. There is really no reason why the inspection community can't create samples which are much closer to being representative of real flaws just by going to their laboratories and creating them. We could produce a fatigue crack at a representative temperature at a representative cracking mode. It's not the real world, but when we can't get the real hardware I don't think we necessarily have to stop at the kind of standard we make in one day or even a week.

D.O. Thompson: I'm not saying you'd have to have all real-world samples, but how do you convince people that things are worthwhile? Real-world samples are not easily available. Certainly you can simulate real cracks or other flaws. The NDE people have done that frequently. You don't need all real-world samples, but you certainly need a few to demonstrate what feasibility is and make a convincing demonstration. Furthermore, the effects of real geometries upon test results must be evaluated. It is very difficult to simulate some of the complex geometries required.

Panel Three

Failure Prediction and Accept/Reject Criteria

Presentor: O. Smith

Panel Members

O. Smith (Chairman)	W. Reimann
C. Annis	J. Richardson
C. Burger	W. Riley
J. Corones	R. Rowand
K. Fertig	F. Taylor
J. Harris	D. Thompson
D. Higton	R. Thompson
F. Morris	R. Trivedi
W. Pardee	H. Weiss
C. Rau	C. Wells

PANEL NUMBER THREE OVERVIEW

- How do accept/reject criteria drive NDE requirements?
- Effects of accumulated damage and performance history.
- The problem of inspection--internal use of extreme value statistics.
- Decision display alternatives.
- Liability considerations

Figure 1

O. Smith: I will give the results of the discussions in the order presented in Figure 1.

We discussed how accept/reject criteria drive NDE requirements, effects of performance history on the formulation of accept/reject criteria, the problem of establishing and using inspection intervals, and decision display alternatives (which is at least partly a human factors problem). I think the answer we came up with to the first of these is that it doesn't at this time. Instead, the reverse is true: in order to establish a real operating RFC system in 1985, NDE capability is going to end up driving the accept/reject criteria to a certain extent. Trade-offs will be made between A/R criteria and NDE. We expect a very confusing and painful learning process during the early phases of RFC. We talked about the possible need to establish accept/reject criteria which will initially allow many false rejects. We would then expect to perform detailed inspection of false rejects through some backup means, to fine-tune and

adjust the system to try to reach an optimum. However, there is another factor working against us: We must learn fast or we will cause an inventory problem. There is a limited number of F-100 engines, perhaps 300 in floating inventory. If we reduce this floating inventory, we will imperil the mobilization readiness of a lot of airplanes. We also discussed the need to develop test and calibration standards; I think we generally agreed that we do need testing and calibration standards. We discussed real cracks versus simulated and manufactured cracks and other defects. We also talked about the need to have surprise tests; that is, discs or other components randomly inserted in the inspection scheme without the knowledge of any of the operators, as a check on the system's accuracy. One thing that came out of the discussion is that there is more to the formulation of an accept/reject criterion than just the crack size. Of course, and as we discussed earlier today, there are other kinds of defects than cracks that are a problem. For example, we have dimensional tolerance difficulties. The formulation of the accept/reject criterion is also going to take into account some information extrinsic to the state of the disc itself as we observe it; that is, some knowledge about its performance history, how it's been used, and some knowledge concerning the results of previous inspections on the disc. The economics of trying to come up with some optimum or desirable accept/reject criterion are extremely complicated. There is a strategic materials issue: if the components of RFC inspection engine pieces become increasingly scarce, then we may be willing to tolerate a much higher failure rate than otherwise. There is also an operational readiness problem: events may occur militarily that will cause us to pass parts that we might otherwise not pass. There are many other issues in formulating A/R criteria than merely the size of cracks.

We also talked about a difficulty that may develop as we do extend part life to two and three times the design life. New failure modes may appear, and they may puzzle the system and us, too. First we discussed the situation in which you have, for example, a ten-times design life disc. It turns out that a ten-times life disc is probably not really going to happen because this would take us well into the Twenty-First Century, by which time the planes will have been scrapped. But it is possible that two- to three-times design life parts may be in service, and we may begin anticipating those. We discussed the fact that as engine life increases, the life of the entire assembly increases, and more parts are going to be RFC candidates. There are also parts which are not highly stressed that we haven't talked much about, but as we develop an engine

that lives at two times the current design life, we may have some other low-stress pieces that become candidates. False rejects are going to be a serious problem. The magnitude of the problem is indicated by the fact that, according to our Pratt representatives, if the design numbers on the F-100 are right, we are going to see only one in a thousand critically flawed discs at the design lifetimes. It's like a needle in a haystack. We may have an NDE threshold far below the critical crack size in order to the the sensitivity we decide to have in the NDE measurements, but we may have a threshold that is so far below criticality that we may be forced to spend a great deal of time, energy, and money winnowing out an excess number of false rejects. It is conceivable that you could have so many false alarms that you would have to resort to some sort of backup system. Again, the issue kept emerging that an inventory has to be kept up. The Air Force will not tolerate a reduction in the inventory of usable engines.

Something else came out in the discussion of the F-100 engine is that it is modular. The modules are swapped around and changed from aircraft to aircraft and fleet to fleet, with the exception of the hot section components, which tend to stay within a given fleet.

We had a side discussion on the role of internal defects in the formulation of accept/reject criteria (Figure 2). What came out of the meeting was a statement that internal defects of interest are bigger than surface defects--perhaps twice as large. The current design trend is to assume internal defects propagate at about the same rate as surface defects, but this is probably not true; internal defects probably grow much more slowly.

AN ASIDE ON INTERNAL DEFECTS

- An internal defect of interest is about twice as big as a surface defect of interest.
- Current trend:
  Assume internal defects propagate same rate as surface defects.
- Most likely:
  Internal defects grow ten times slower
- Conclusion:
  We have to look for them, but probably not a serious problem.

Figure 2

The consensus of the group is that internal defects are important and we have to look for them. They can become a problem but the detection, sizing and characterization of internal flaws is not as critical a part of the RFC accept/reject decision methodology as the surface flaws.

We had extensive discussion on the second topic, the effects of accumulated damage. We talked about the possibility of measuring what I would call nondiscrete accumulated damage--a bulk measurement of accumulated damage in a disc, perhaps something like the fatigue data obtainable with the acoustic harmonic generation technique. Although it is a good idea and would be a useful measurement, there was a great deal of scepticism in the group about whether such techniques would be practicable in time to be incorporated into a first general RFC system. What we do have available are engine performance data. At present we have some information on throttle cycles.

We talked about the possibility of expanding the performance history data base. There is some British experience in fairly detailed monitoring of what goes on in an aircraft engine: r.p.m. levels, how much time an engine spends at a given r.p.m., temperature, and fuel-flow monitoring. The consensus that came out of the group was that the Air Force would consider this kind of data collection to be an unacceptable burden, that the data management task was too great. It is probably not realistic to try to formulate an RFC decision methodology which relies on making use of more engine performance history data than are currently available. As part of this, also, we talked about short crack fracture mechanics; that is, the ability to predict the propagation of cracks that are shorter than or about grain size in a given material. They propagate differently from larger cracks. For high-nickel alloys, we thought that it would be interesting to know, but that short crack fracture mechanics is probably not necessary to make a lifetime prediction and to establish some kind of crack length accept/reject criteria; chances are that if you can see a crack in an encased disc, you have a problem. In titanium alloy components, we generally thought that it would be necessary to have some ability to predict crack growth rates for short cracks, that if we didn't develop that capability we were going to be using an overly conservative reject decision point. A summary of this discussion is given in Figure 3.

EFFECTS OF ACCUMULATED DAMAGE ON SUBCRITICAL FLAWS

- Measure nondiscrete accumulated damage
  - A noble goal, but not in 1985 (e.g., acoustic harmonic generation).
- Engine performance history next best
  - Some data available (cycles).
  - Expanding data base unacceptable burden
- "Short Crack" Fracture Mechanics
  - Probably not necessary in high-nickel alloys
  - Probably very useful in Ti.
- Conclusion: History data base limitations will make history second order effect (in real system)
- Short crack mechanics needed

Figure 3

We also talked about the desirability of trying to establish variable or optimal intervals (Figure 4). However, it turns out that the inspection intervals are going to be predetermined for us. They are already set by existing maintenance practices. There is very little hope of trying to come up with variable inspection periods for different disc components for engines out of different aircraft that may show trouble developing on successive inspections. The group's conclusion was that for political and logistics management reasons the inspection intervals will be prespecified when the system comes on-line in 1985. It is possible as RFC gains status and becomes generally accepted that the RFC optimization criteria may begin to set the inspection intervals. Again, this is a tremendous logistics and data management problem for the Air Force, which the Air Force is not currently ready to assume. We strongly recommend that our system should have a data base which is able to remember what a certain disc looked like at a previous inspection; this would allow us to do several things. We could do templating; we could do a comparison between a previous inspection and current inspection to see if there are differences. It would also provide long range consistency verification, and it would allow us to pay special attention to suspicious regions.

There was also an extended discussion on the use of extreme value statistics for consideration of the extreme end of the failure probability distribution curve. In this range the probability of a failure event is 10^{-3} to 10^{-4}. The difficulty is that it is impossible to take enough data to fully specify the failure probability curve; you could never fail enough discs to establish

all possible correlations between defect characteristics and failure life. The recommendation developed in the meeting was to develop a distribution model, based on failure mechanisms, and then to validate the model by not disproving it; that is, use relatively few experimental points to validate the center portion with a reasonable amount of data, and then use some relatively new statistical techniques in order to extrapolate the middle range out to the extreme values. Some people argue that this all boils down to Bergson's interocular trauma test, which means if the data hit you between the eyes they must be right. The outcome of all of this is that there are well-known techniques for dealing with extreme value data problems and we are going to have to apply them to the RFC situation. We are never going to know all there is to know.

The fourth topic--decision display alternatives--was dispensed with quickly. The consensus of the group was that there is no alternative to a simple yes-no binary display of test results. The information content that the operator has to deal with must be reduced to this level--an unambiguous binary signal. A summary of discussions on the topic of variable or optimal inspection intervals is given in Figure 4.

INSPECTION INTERVALS

- Inspection intervals already set by existing maintenance practice
- As RFC gains status, it may set intervals
- Variable intervals very tough logistics problem
- System should be able to remember what a disc looked like in a previous test
  - Templating
  - Consistency verification
  - Special attention to suspicious areas
- We're worried about extreme value events--$p=10^{-4}$?
- Taking enough data to "know" we're right is ruinous
- Approach
  - Develop a mechanism-based model
  - Validate model (at least don't disprove it) with reasonable amounts of data
  - Extrapolate model to "tails"
- Relatively new statistical techniques in hand to do this; e.g., Bergson's Interocular Trauma Test

Figure 4

In discussion of the fifth and last point, the liability problem, two issues were involved: If you have RFC you may get into trouble, and if you

don't have RFC you may get into trouble. The consensus of the discussion was that liability is not thought to be a really serious issue in military applications. Now, there are some exceptions to that. If RFC for some reason results in a higher failure rate, then obviously someone is going to have to pay the cost of those failures. It will probably be the Air Force. If RFC follows the pattern of other military hardware development programs, it will evolve slowly into commercial applications, and during that relatively slow evolution it will be perfected. By the time RFC is actually applied to commercial aircraft engines, the risks of applying it will be very low. There is a difficulty, however; it is possible to imagine a scenario in which the mere existence of a workable inspection and lifetime prediction technique of greater accuracy than that which is used in commercial aircraft engines is applied to military engines. The existence of that technique may be used as evidence that the commercial aircraft engine manufacturer is not doing all he could to ensure the safety of his product. There was a recent case in California in which the manufacturer had shifted to him the burden of proving that he could not have designed a safer product. Court decisions such as that make manufacturers very wary of what the future may hold. In light of all the things we are doing in RFC, the liability community is going to need a new definition of a defect. If our NDE techniques get good enough, pretty soon we will know, and will have records of knowing, that every component in an engine has some anomaly in it; obviously, those will not all be defects that represent a justifiable reason for taking a component out of service. But judges and juries and the law tend to lag a few generations behind the technologists. We feel, on balance, that RFC should ultimately reduce the liability exposure of everyone involved. But there are some mid-term difficulties that could arise. These points are summarized in Figure 5.

LIABILITY

- Liability is not thought to be a serious issue in military applications.
- RFC will evolve slowly to commercial application. Risks of applying will be low.
- Liability community will need a new definition of "defect."
- RFC should ultimately reduce liability exposure.

Figure 5

That, I think just about covers what we discussed.

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Panel Three

DISCUSSION

D.O. Thompson: Wally Reimann has to leave shortly and has a couple of comments that he would like to make.

W. Reimann: Thanks very much. Before I start, I would like to say how much I appreciated Ora's pitch, because I sat through that yesterday and I was very curious as to what we discussed, too. And this morning I found out. But I really just wanted to make a couple of comments on the last day's efforts. I'd like to go back to some things that Mike said on our first morning, when he posed the question, "Show me why retirement for cause won't work." Now that sometimes can be a very interesting intellectual exercise and sometimes it can even be valuable, but it can also be very dangerous. I am reminded of a situation that took place in 1942 when President Roosevelt called together a very high-level panel to make some decisions on some activities. This panel deliberated for about six months and finally went back to the President with a very emphatic recommendation that the U.S. should not spend its technology dollars and manpower in trying to develop a jet engine because the probability of successfully applying a jet engine to military or civilian aircraft was so low as to make the thing virtually worthless. Fortunately for a great many of us in this room, that recommendation was not accepted. But this story points out that it is very easy to get bogged down in the negative in looking at problems like this. Over the last couple of days I have had the impression that we were leaning that way, and if the question had been posed, "Tell me when we can do retirement for cause," I was a little afraid that the answer would be "Probably around 2050." This morning, however, I have been very pleased to see that this hasn't developed.

I think the reason we are all here is that we don't want to go into this thing with blinders on and say, "Retirement for cause is going to work in 1985 and that is all there is to it." We are trying to approach it in a very positive way, and I feel as I have listened to these summaries this morning that what we are getting are very positive comments in terms of what we can do and in terms of where the weaknesses are and so on. I, for one, am very appreciative of that. I think there are opportunities for things that can be done over the next few years, and I think probably should be done. I apologize for having to dash off to the airport, but I want to thank everyone for their participation, at least on the part of the Air Force, and I think we have had a good meeting.

D.O. Thompson: Thanks, Wally. Let's return to some of your questions and answers now, Ora.

B. Tittmann: You talked about liabilities. Do you think that the burden of liability may, in fact, shift to those people or companies who are carrying out the RFC? Is that a realistic conjecture?

O. Smith: It's a possibility, especially in the commercial field. Normally, most aerospace companies which do hardware development business for the government have in their contracts indemnification clauses under which the government agrees to hold the manufacturers harmless, to indemnify them, for any successful suits against the manufacturer which arise out of the government's use of their product. The situation is different, however, for the manufacturer of a component of a commercial aircraft. Aircraft are regarded as inherently dangerous things in the eyes of the law, and any manufacturer of a piece of an airplane has a considerable liability exposure every time there is a crash. It is not necessary in most airplane crashes to show that the manufacturer was negligent. There is a theory of strict liability, which goes like this: In the case of injury that is caused by something which is a dangerous thing, such as an airplane or a part of an airplane, all you have to show is that there was a defect in it when it left the manufacturer's hand and the defect caused the injury. You don't have to show anything about due care, that the manufacturer was negligent, that he did something out of his way to cause this defect to happen. If, for example, you had a perfectly good RFC-inspected turbine disc, and in your data banks you had a lot of information that said there are little one-mil type cracks in this thing and they are everywhere but they are not a problem, and there was a crash, you could see an added risk exposure placed on that manufacturer; he has a data base which can be discovered and admitted as evidence.

J. Rose: Coming back to the liability, I was really a bit perplexed by your last comment and I would like you to explain it, which is that RFC should reduce the overall liabilities exposure. I don't understand why.

O. Smith: Well, if RFC works, presumably one of the effects it might have is to reduce early failures. To the extent it does that, it could reduce liability exposure. It is conceivable that it might also reduce liability exposure by allowing the manufacturer to show that he is more safety conscious, is using improved techniques, especially in conjunction with the uniform product liability law which is being considered now. This law proposed to change the definition of a defective thing to one which deviates from the manufacturer's

specification. With RFC technology, you would be better able to show that you produced a piece which did not deviate from your specifications.

J. Richardson: From what you said earlier, I assume that the RFC system may reduce your exposure to punitive damages but not necessarily the exposure to liability for a particular failure.

O. Smith: It might reduce your exposure in a particular failure in conjunction with this proposed revision in product liability law. The way things stand right now, it probably wouldn't.

J. Richardson: I was assuming that when your punitive damage aspect comes up, would you say that you had a procedure that was pretty much the standard for the industry for accepting and rejecting parts and everything according to acceptable probabilistic standards, etc.?

O. Smith: Punitive damages don't get assessed too often in product liability cases. You have to show that the manufacturer acted with callous and reckless disregard for human life.

F. Morris: I don't think the consensus is really in on whether a statistical short crack model in titanium will be more or less conservative than continuum fracture mechanics. AFML has a program, either in progress or to start shortly, to test that thesis.

O. Smith: I made the assumption that they would be less conservative. I guess that is not true.

F. Morris: I don't think that answer is in yet.

J. Harris: I'm a program manager, and I'm supposed to make this happen for the F-100 in 1985. Quite often, I find that I don't speak the same language as several of the other people who are in this same business. "You don't understand my problem, I've got to make it work." Then you come back and tell me, "But you don't understand some of the questions that have to be resolved." The only way to obtain that understanding is through active communication. I would point out that one of the problems in active communication is the fact that I work for a company, as several of you do, and that company has certain things which it considers sensitive to its well-being in the future. We are attempting to work out the details to allow a much more frequent, intimate interchange. I think it is vital. From my point of view, you guys have got to learn what's in the real world, and from your point of view I must learn that the problems aren't as simple as I sometimes think they are. This type of exchange is a means of getting there. I hope it can be frequent. I hope we can work out the details. We found it very helpful to listen to other opinions and views.

Hopefully, by 1985 we will all be on the same wavelength and we will all be very happy. I appreciate this opportunity for us, particularly from Pratt-Whitney, to have participated in this. Thank you.

R. Elsley: When Panel Three was talking about the significance of internal defects, were they thinking more about subsurface or deep internal defects?

O. Smith: My recollection is that we talked about both, and agreed that defects close to the surface were a bigger problem than defects that were not.

J. Richardson: I also remember that in many situations where we have bending stresses applied, the stresses are strongest on the surface of the weakened side. That was one of the reasons. Not the reasonable one, of course.

C. Rau: My answer to that would be yes, I think so. We decided that near-surface defects would be considered as surface defects, of the same order of importance. If you have a near-surface crack, you develop very large stress intensity factors in a small area between the crack near the surface and the surface itself, much larger than you would have for a surface-breaking crack or a crack far away from the surface.

L. Kessler: I have a general question. It has to do with the performance history of the component. It seems that at present it is before retirement for cause procedures are being used that parts are retired based on their calculated lifetimes, or some fraction of it, and it seems that the performance histories are integral parts of the discussions here. If we are to rely on the NDE methods for determining whether or not a part is good, we really don't need the performance history. I wonder if there might be any comments about that.

O. Smith: We talked about that at some length in Panel Three, and I guess the conclusion was that if NDE techniques were perfect you wouldn't need to know the performance history. Since they are not, it gives a particular importance to the performance history as a source of extrinsic data about the state of that disc. The problem is that we don't seem to have any convenient way to collect the detailed and comprehensive performance data to do a really good job of incorporating that. I believe someone in the panel summarized it by saying that basically what we are going to have to do is look forward and not look back; anything we are able to obtain from looking back will be a benefit, but we can't really count on it. Unfortunately, we're not perfect.

F. Morris: Another aspect of your question, though, is that remaining lifetime, once a crack has been detected, is sensitive to the performance history, and so if one wants to make a prediction of the statistical probability of failure with additional usage, then it becomes important to have that number.

But I think the panel concluded that at least for the 1985 time frame, the models weren't there to make that kind of prediction work and that would be something one could contemplate applying much further downstream.

D.O. Thompson: Any other general questions? If not, I want to thank you all for participating. I feel we have had a good discussion. As Mike said earlier in the meeting, and this meeting is under his sponsorship, there are many elements in RFC. It is about as multidisciplinary as NDE itself, and there are many ramifications to it. I concur with some of the comments made earlier that we do need more interactive workshops. Again, thank you for your participation.

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LIST OF ATTENDEES

DARPA/AFML
RETIREMENT FOR CAUSE WORKSHOP
Iowa State University and
Ames Laboratory, USDOE

October 27-29, 1980

J.D. Achenbach
Northwestern University
Evanston, IL

R.C. Addison
Rockwell International Science Center
Thousand Oaks, CA

Laszlo Adler
Ohio State University
Columbus, OH

C.G. Annis, Jr.
Pratt & Whitney Aircraft
West Palm Beach, FL

B.A. Auld
Stanford University
Stanford, CA

A.J. Bahr
SRI International
Menlo Park, CA

Simon D. Bennett
Stanford University
Stanford, CA

Donald Birx
Systems Research Laboratory
Dayton, OH

Otto Buck
Ames Laboratory
Ames, IA

Michael J. Buckley
DARPA
Arlington, VA

C.P. Burger
Iowa State University
Ames, IA

J.S. Cargill
Pratt & Whitney Aircraft
West Palm Beach, FL

A. Colej
General Electric Company
Schenectady, NY

J.P. Corones
Ames Laboratory
Ames, IA

Mark Davidson
Ames Laboratory
Ames, IA

Roger DeWames
Rockwell International Science Center
Thousand Oaks, CA

James Doherty
Pratt & Whitney Aircraft
West Palm Beach, FL

Richard Elsley
Rockwell International Science Center
Thousand Oaks, CA

Ted Fecke
Wright Patterson Air Force Base
OHIO

K. Fertig
Rockwell International Science Center
Thousand Oaks, CA

D.K. Finnemore
Ames Laboratory
Ames, IA

Amos Greer
Southwest Research Institute
San Antonio, TX

R.S. Hansen
Ames Laboratory
Ames, IA

John A. Harris, Jr.
Pratt & Whitney Aircraft
West Palm Beach, FL

Derrick R. Higton
British Embassy
Washington, DC

Larry Kessler
Sonoscan Inc.
Bensenville, IL

Tom Kincaid
General Electric Company
Schenectady, NY

K.L. Kliewer
Ames Laboratory
Ames, IA

K.M. Lakin
Ames Laboratory
Ames, IA

John McClelland
Ames Laboratory
Ames, IA

Fred L. Morris
Rockwell International Science Center
Thousand Oaks, CA

Joseph A. Moyzis, Jr.
Wright Patterson Air Force Base
OHIO

Vicki E. Panhuise
AiResearch Manufacturing Co.
Phoenix, AZ

Bill Pardee
Rockwell International Science Center
Thousand Oaks, CA

John Prati
Teledyne CAE
Toledo, OH

Charles Rau
Failure Analysis Associates
Palo Alto, CA

Wally Reimann
Wright Patterson Air Force Base
OHIO

John M. Richardson
Rockwell International Science Center
Thousand Oaks, CA

William Riley
Iowa State University
Ames, IA

R.R. Rowand
Wright Patterson Air Force Base
OHIO

Lester W. Schmerr
Ames Laboratory
Ames, IA

Ramesh Shankar
Tetra Tech, Inc.
Arlington, VA

Shen-ann Shei
Ames Laboratory
Ames, IA

Kenneth D. Shimman
Wright Patterson Air Force Base
OHIO

Ora E. Smith
Rockwell International Science Center
Thousand Oaks, CA

Frank Taylor
Systems Research Laboratory
Dayton, OH

Arthur L. Thompson
General Electric Company
Cincinnati, OH

Donald O. Thompson
Ames Laboratory
Ames, IA

R.B. Thompson
Ames Laboratory
Ames, IA

Bernie Tittmann
Rockwell International Science Center
Thousand Oaks, CA

Vasundara Varadan
Ohio State University
Columbus, OH

Vijay Varadan
Ohio State University
Columbus, OH

Harry Weiss
Iowa State University
Ames, IA

Clifford H. Wells
Southwest Research Institute
San Antonio, TX

Loren Zachary
Ames Laboratory
Ames, IA

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